Patterns of Knowledge Communities in the Social Sciences

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ABSTRACT

THE STUDY OF SCIENCE AND SCIENTIFIC COMMUNITIES is dominated by philosophies and sociologists. These disciplines naturally take different approaches to the subject, the one epistemological and the other sociological. While recognizing the role of society in shaping science, this article emphasizes the way that the epistemology of science influences scientific society. The epistemological status of various scientific discourses also shapes scientific communities. Discourses about methods have different effects on communities than discourses about theories; positivist discourses and nonpositivist discourses also shape communities differently. The best way to think about science and scientific communities is a dialogue between two hybrid approaches—i.e., a social epistemology and an epistemological sociology. Each presents some challenges to information science.

INTRODUCTION

Knowledge is found in communities built by individuals. Our efforts to systematize, categorize, or reorganize that knowledge must consider not only the individual knower but also the knowledge communities. In other words, studying knowledge presents a sociological problem in addition to an intellectual or philosophical one.[1] For this reason, most contemporary studies of science treat science *purely* as a sociological issue.

In contrast to this literature, it will be argued here that knowledge communities present not just a sociological problem. The substance of science, and what is labeled here as the "epistemology" of science,[2] affects the pattern by which scientific knowledge is organized. In particular, the epistemological status of a scientific discourse shapes the sociological struc-

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ture of a scientific community. To understand knowledge communities, then, we need an epistemological sociology (ES) of science. This approach joins both the social and intellectual reasons why knowledge communities look the way they do.

While a polemical argument is made for such a sociology elsewhere (Pahre, 1995), this article will evaluate both the sociological and the epistemological reasons for the pattern of scientific organization, generally with reference to the social sciences. Four perspectives toward the problem of understanding disciplines and cross disciplinary research are discussed: (1) a purely epistemological approach, (2) a purely sociological approach, (3) a social epistemology, and (4) an epistemological sociology. These perspectives are lenses through which we can see different aspects of the organization of knowledge. Because neither of the two pure approaches is adequate for understanding how knowledge is organized, our studies of disciplines must be interdisciplinary.

Within this general project, special attention will be given to the twin issues of boundaries and boundary crossing. After all, being a community entails having boundaries of some sort, whether they take the form of walls or transitional zones between one community and another. Information science must deal with both intraboundary and interboundary communities. For instance, cataloging is an attempt to get the boundaries right, while reference librarianship must inevitably confront boundaries that are useful for one purpose and yet hinder the information search at hand. This is especially important because innovative knowledges are most likely in exactly those areas that are most difficult to classify and organize (Dogan & Pahre, 1990).

Like other contributors to this issue (see Dogan's and Klein's articles in this issue of *Library Trends*), the goal here is to describe patterns of knowledge creation today and not to propose how information science can meet the needs of the knowledge creators (for a discussion of this topic, see Palmer's and Searing's articles in this issue of *Library Trends*). Simultaneously, the pattern of knowledge creation and organization has implications for information science that will be touched upon throughout this article. Where there are epistemological reasons for a given pattern of scientific organization, then these presumably provide us with good reasons for organizing information services around them. Where scientific communities are organized for (nonepistemological) sociological reasons, the solution to problems of information will be less clear cut because intellectual and social principles of organization do not coincide.[3]

A Purely Epistemological Approach to Knowledge Communities

For the most part, university curricula and administrative divisions assume the existence of coherent fields of knowledge and groups of fields within identifiable boundaries. The naïve view is that these fields and the boundaries around them are found in nature: the objects of natural science are distinct from those of social science, pure science is epistemologically distinct from applied science, and scientific knowledge is distinct from nonscientific knowledge. These are "epistemological" claims about scientific communities since the alleged division between pure science and applied science rests on the difference between an epistemology appropriate to the search of knowledge for its own sake as opposed to an epistemology for seeking knowledge as a means to another end.

The existence of epistemological distinctions between kinds of knowledge or between the disciplines also finds more sophisticated expression among philosophers. For instance, Steve Fuller (1988) argues that disciplines are bounded by their procedures for adjudicating rival knowledge claims (p. 191). In particular, a discipline's argumentation format restricts word usage, whether justification may rely on reason alone or must use technically aided perception and so on. Presumably, different procedures cluster into disciplines, providing an epistemological explanation for the pattern of disciplinary organization. This is essentially Julie Thompson Klein's (1990) definition of a discipline: "[T]he tools, methods, procedures, exempla, concepts, and theories that account coherently for a set of objects or subjects" (p. 104).

Still, many have rightfully asked whether the alleged boundaries between science and nonscience, pure science and applied science, or natural science and social science, can be philosophically justified. The boundary between science and pseudoscience, for instance, does not rest on the normal demarcation criteria that many scientists believe it does. Paranormal research programs present models with testable hypotheses, for instance, while those who accuse such research programs of fraud do not themselves meet the traditional standards of "science" (Collins & Pinch, 1979; Pinch, 1979). It is also notoriously difficult to distinguish pure and applied science because: "The *intellectual* procedures adopted in pure and applied research are frequently indistinguishable and the scientific results often identical" (Mulkay, 1977, p. 95). As for the natural and social sciences, I cannot imagine criteria that would classify, for example, Jane Goodall as a natural scientist studying unknowing objects and quantitative economists as social scientists studying knowing objects.[4]

There are good reasons why such attempts to define boundaries should fail. These attempts are a variation of foundationalism within philosophy more generally, and analogous to the search for first principles on which to ground a (normative) philosophy of science. The trend of twentieth-century philosophy has, of course, been away from foundationalism. Among those reasons relevant here is that foundationalist philosophies of science unavoidably rest on empirical claims and prelogical judgments in order to justify their prescriptions for how scientists should engage in science (e.g., see Giere, 1985). For instance, crude empirical claims about the "success" of physics in explaining the world have had an enormous effect on notions of what a normative philosophy of science must allow (or prescribe).[5] Judging by past efforts, a philosophy absent from some such nonphilosophical foundations is impossible.

As an empirical matter, foundational explanations would expect a relatively static organization of scientific disciplines, since the justifications for dividing the disciplines are, by definition, unchanging. This is very much at odds with reality,[6] where interdisciplinary centers, institutes, programs, and even colleges and universities abound (Dogan & Pahre, 1990; Klein, 1990). Pure philosophy would reject such an empirical test as illegitimate, of course. Yet, if some empirical claims inevitably lie behind any philosophy, then alternative empirical claims about the nature of disciplines do present a legitimate criticism of even the purest prescriptive philosophy of science.

While we should be suspicious of claims that disciplinary divisions exist in nature, they certainly do seem to influence the definition of disciplines. Divisions between "pure" and "applied" fields are ubiquitous in the physical sciences (science versus engineering), biological sciences (biology versus medicine), and social sciences (economics/business administration, sociology/social work, political science/policy studies). One reason these divisions exist is that asserting epistemological divisions is an important part of scientists' "boundary work" (Gieryn, 1983). Thus, it is not too surprising that this apparently "intellectual" division probably makes more sense sociologically. Pure scientists produce for an audience of other researchers and are supposed to choose topics based on scientific "significance" (however defined). Applied researchers produce for a nonresearcher audience things of practical usefulness (or things that a nonresearcher audience is willing to fund as if practically useful).

These audience effects shape the communities and their boundaries.[7] This sociological reality of science presents a serious challenge to any purely epistemological attempt to understand disciplines and other forms of knowledge communities (e.g., see Becher, 1990; Campbell, 1979; Gieryn, 1983; Huber, 1990; Pinch, 1990).

A Purely Sociological Approach to Knowledge Communities

If epistemology cannot explain them, then perhaps a sociological approach is the best way to understand knowledge communities. Unfortunately, the classical sociology of science was more a sociology of *scientists* than a field which problematizes knowledge. For Robert Merton and his disciples (i.e., Merton, 1973; Ben-David, 1973; Zuckerman, 1977), science is a particular form of social activity, where (in a Parsonian way) existing norms structure the roles filled by individual scientists coming from a variety of social backgrounds and forming various kinds of networks. The internal allocation of rewards in science exerts a powerful influence on scientific recruitment, socialization, and knowledge production, as do professional networks, hybrid fields and scholars, journals, reading and citation patterns, or the (dis)integration of scientific specialties (i.e., Crane, 1972; Mullins, 1973; Chubin, 1976; Mulkay, 1977; Dogan & Pahre, 1990). External belief systems, such as Puritanism or democracy, might also help or hinder the spread of science by supporting certain norms (Merton, 1938/1970, 1973).

This approach usually does not seek to explain the substance of science, generally assuming that science progresses, and that each generation provides a successively better account of reality. Their inattention to the substance of science stems in large part from a belief that "true" scientific beliefs are best explained by their truth; only "false" beliefs need sociological explanation. Laudan (1977) calls this the arationality assumption, by which he means that "the sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits" (p. 202). This is a profoundly ahistorical assumption, for it means that any 1950s-era sociology of knowledge would have been precluded from studying contemporary geology, while those writing after the tectonic revolution can provide a sociological account of geology as it was in the 1950s. Now, of course, those in the sociology of knowledge field are (temporarily) prohibited from studying scientists who believe in plate tectonics. Given such problems, this author rejects the claim that only false beliefs need explanation. In any case, it is more interesting to study the sociological conditions for "true" claims and the conditions affecting the variation between truth and falsity in knowledge claims.

While interesting, it is not immediately obvious why a sociology of "true" belief systems is possible. There are two major reasons why a sociology of knowledge is both possible and interesting—one epistemological and the other sociological. The epistemological reason is the Duhem-Quine thesis that scientific theories are underdetermined by the evidence because more than one theory fits any given set of evidence. "Facts," too, are equivocal, embedded in a particular research program or paradigm. No apparent anomaly can destroy a scientific research program since scientists may make the anomaly disappear by distrusting scientific instruments, restricting the domains of theories, and so on (Kuhn, 1962; Lakatos, 1970). This thesis implies that factors other than evidence—presumably including social factors—determine the content of scientific theories.

Karl Mannheim (1936) first noticed the sociological reason why a sociology of knowledge is interesting. He indicated that granting the truth of any knowledge claim benefits some people at the expense of others. Since any epistemic claim benefits some at the expense of others, the process of epistemic justification is really just another way of distributing power. This is especially clear when we consider how certain domains of socially important knowledge, such as law or medicine, have been delegated to experts (Fuller, 1987). The creation of disciplines, too, served various social interests at particular moments in time (see Wallerstein, 1991). These disciplines and professions are far from epistemically "efficient"; after all, if one could start over and design intellectual boundaries for the current corpus of knowledge, surely one would not choose precisely the set of boundaries that we have today (Fuller, 1988, pp. 195-97; Whitley, 1986).

Where Mannheim (and his followers) erred was in assuming that, because someone benefits from the acceptance of any knowledge claim, this distribution of power is sufficient to explain why a knowledge claim is accepted. They do not consider the possibility that there are different kinds of interests in a given knowledge claim: the material interest of, for example, capitalists on the one hand, but on the other hand the contending professional interests of scientists who have staked positions on opposite sides of a particular knowledge claim.[8] Similarly, disciplines persist independent of the "external" society's class interest, in part because they engender material and professional interests in their continued survival—a fact evident whenever a university tries to abolish, for example, its geography department.

Even if we grant these weaknesses in the sociologists' interpretation, scientific claims to a monopoly over certain truths cry out for challenge. The sociology of science has met this challenge, providing a thorough critique of the "myth" of scientific rationality, technical competence, and social authority. Ethnographic studies of science (see Knorr-Cetina & Mulkay, 1983 for an overview) have demystified the process by which scientists construct their knowledge claims by watching scientists create, construct, or find evidence and then seeing how they choose among alternative explanations for the evidence they have. What matters, they argue, is not the relation between science and external reality, but the process of reflexive fabrication that yields science (Knorr-Cetina, 1983, pp. 118-19).

Such work has produced a new approach, a constructivist sociology (CS) of science.[9] Scientific beliefs are socially constructed, and changes in scientific beliefs arise from social and social psychological factors—they do not reflect successively better models of "reality." CS has dominated recent contributions to the sociology of science (i.e., Knorr, 1981; Knorr-Cetina & Mulkay, 1983; Latour & Woolgar, 1979; Mulkay, 1979; Woolgar, 1981). Even Mertonians now find parts of the constructivist position persuasive (i.e., Cole, 1992).

Constructivists argue that the boundaries between disciplines are important as the objects of political conflict, broadly defined. It is not by accident that physicians defend schools of medicine from schools of public health, that physicists look down on engineers and economists, or that national academies of science are reluctant to admit social scientists. In the academic context, boundary struggles decide who controls the structure of the curriculum, dissertation writing, tenure and promotion decisions, journals, university presses, and external funding sources (see Klein in this issue of *Library Trends*). There are economic, political, and professional motives for all these boundaries.[10]

Boundaries between academic divisions also make sociological sense. For instance, Pierre Bourdieu (1975) argues that the division between the natural sciences and the social sciences may be the natural outgrowth of class (or elite) domination:

> whereas the dominant class grants the natural sciences an autonomy corresponding to the interest it finds in the economic applications of scientific techniques . . . the dominant class has no reason to expect anything from the social sciences—beyond, at best, a particularly valuable contribution to the legitimation of the established order and a strengthening of the arsenal of symbolic instruments of domination. (p. 36)

Thus, the divisions must be understood in terms of their social purposes and not as the result of epistemological differences.

For all its insight, constructivist sociology is not without its problems. To begin, we note that, because multiple constructions are possible and the ultimate construction is socially determined, constructivism "is based upon a relativist epistemological position and the argument that nature has very little, if any, influence on the development of the content of science" (Cole, 1992, p. ix). Karin Knorr-Cetina (1983), a leading proponent of CS, bluntly claims that the scientific laboratory is "not an establishment designed to mimic nature" (p. 135). Harry Collins (1981) agrees that "the natural world has a small or non-existent role in the construction of scientific knowledge" (p. 3). Going further, it does not matter whether reality exists, for reality does not constrain our accounts of it (Woolgar, 1983).

Some will find this relativism objectionable on its face, but let us limit ourselves to those objections relevant to the sociology of science. First, the claims of constructivist sociology become more difficult to defend when we look at knowledge claims accepted across many cultures or time periods. There is every reason to believe, then, that CS and all sociological approaches exaggerate the social explanation for scientific beliefs held in many places and at many times. One such belief is that science naturally falls into recognizable fields or disciplines. Because there is a justifiable foundation for this belief, certain principles of disciplinary organization and patterns of cross disciplinary organization make sense on epistemological grounds.

Epistemology constrains cross disciplinary synthesis, a topic that constructivist sociology has not yet examined (Goldman, 1995). Like most sociologies of knowledge, CS examines the construction of scientific fields or continued knowledge production within boundaries. Yet these are exactly those cases where sociological variables are most important, because knowledge production in a given scientific field takes place within an established social structure. Inside a discipline, existing administration, professional associations, professional socialization, and gatekeeping by the powerful all affect knowledge production.

Since they have studied disciplinary knowledge, it is not surprising that contemporary sociologists have taken a skeptical view of science. Their conclusions might be different if they were to study the destruction of scientific fields, or processes such as boundary crossing, the migration of subfields, the reorganization of knowledge, or the partial destruction of two fields that later merge and create a new hybrid body of knowledge. Reality seems to play an important role in these cases.

Another weakness of the constructivists is that constructivist sociology cannot explain why anyone takes science seriously. Fairness requires that we grant our subjects at least as much understanding of their interests as we claim for observers such as us. If sociologists can see that reality does not constrain scientific narratives, then the producers and consumers of those narratives should realize the same thing. This insight may not stop cynical knowledge producers from producing science that they know to be unconstrained by reality, but it should prevent governments, foundations, and other scientists from believing any of the accounts thus produced.

As this point suggests, constructivist sociology must deny the norms and belief systems of science. This is odd for a relativist position, which is value neutral with respect to every other kind of belief (Pahre, 1995). This denial can even help interrogate constructivism, for CS cannot explain its own efforts to develop a "true" account of how science is constructed. If reality does not constrain scientists' accounts of reality, then so too science must not constrain sociologists' accounts of science. If this is true, then why do constructivist ethnographers study real scientists in Jonas Salk's lab?

Finally, and most relevant for the subject of this issue, constructivism makes a mockery of information science. First, following constructivist sociology would require information providers to achieve a heroic level of cynicism. It asks them to provide information with which scientists can construct accounts of reality while also asking them to believe that this information will not constrain scientists' accounts of reality. Second, CS implies that there will be no systematic relationship between patterns of information organization and patterns of scientific organization (since scientific organization is not predicated on reality or evidence about reality).

There is, of course, a middle ground position that allows for a meaningful constructivist sociology, though it is different from existing versions.

Goldman (1987) argues that "while students of science and culture may properly abstract from the truth of the ideational contents they study, they do not and should not extrude the question of truth from their own propositions about the growth, prevalence, and extinction of people's ideational contents" (p. 126). In other words, CS should admit that it seeks "true" knowledge claims about knowledge, and that real knowledge communities do play some role in the construction of knowledge about knowledge. When we grant this, then CS must also allow for the possibility that reality exists and affects scientists' accounts of it—i.e., science is not purely a social construction. At the same time, society does shape science. This brings us to our next question: Given that society constrains our accounts of reality, how do we ever produce true knowledge?

A Social Epistemology

The previous section argued that sociologists must take note of the role that reality plays in shaping scientific narratives. Yet, as the first section argued, reality and other epistemic concerns do not, by themselves, explain knowledge and knowledge communities. Social forces, too, shape our accounts of reality. Social epistemology examines this problem, analyzing those social structures that are more (or less) likely to encourage conversion to "true" scientific beliefs than are other social structures.[11] This project only makes sense if there is a (partially) knowable reality against which to evaluate knowledge claims. Since the social epistemology project is in part empirical, it is no surprise that it is grounded in something other than a classical epistemology. A "naturalized philosophy" (Quine, 1969; Giere, 1985) or an "evolutionary epistemology" (Campbell & Paller, 1989) are common starting points.

While a social epistemology does not seek to explain the shape of knowledge communities, it does highlight certain patterns or biases in the knowledge held by communities; an epistemological sociology can then show how these patterns help shape the community. To the extent that scientists find themselves in communities that foster the pursuit of true beliefs, we should see an explosion of knowledge. The epistemological characteristics of this knowledge should affect the form scientific communities take. To the extent that scientists are in communities whose organization hinders the pursuit of true knowledge, epistemology will play less of a role in shaping communities. Presumably, social factors will be more important.

The problem of nonmainstream research is a straightforward example of how this might work. Peer review authorities seem to be systematically biased against nonmainstream work in general and academic whistle-blowing in particular (see Moran & Mallory, 1991). This conflicts with scientific norms and biases knowledge production. This bias, in turn, makes it look as if mainstream approaches are better explanations of the world than they really are. This apparent "success" of the mainstream gives greater intellectual authority to powerful scientists in the mainstream, helping them retain control of gatekeeping roles such as peer review authorities. Mainstream science advances within these limits but is inevitably incomplete.

Another example is the existence of disciplinary boundaries. To defend boundaries, scientists highlight certain claims while obscuring others. For instance, a cultural anthropologist studying reciprocal gift giving will tend to downplay forces of supply and demand influencing the relative value of different goods in order to emphasize the social positions of the two participants; an economist studying the same phenomenon will downplay or ignore the social position of the people involved and highlight the relative prices of the goods.

In such cases, what one discipline ignores are exactly those processes that are part of other disciplines, a process known as "ontological gerrymandering" (Pawluch & Woolgar, 1985; Fuller, 1988, p. 197). One example of how boundaries exclude extradisciplinary factors is the sociology of science itself (compare Delamont, 1987). Mertonian sociology, following Parsonian sociology, showed a concern for norms, socialization, and social structure as influences on science. Challenges to this position became increasingly important with the work of Thomas Kuhn (1962), a physicist turned historian of science, whose researches fall outside sociology proper.[12] Ethnomethodology, a sociological field with roots outside the discipline (Mullins, 1973), also became an important source of new approaches to the sociology of science and a major influence on constructivist sociology. One effect of these new approaches was that the sociology of science excluded normative research, leaving that to the philosophers. The philosophers, for their part, had turned away from a concern with real knowledge communities. In short, "[a]n implicit agreement seems to have been made to let the sociologists concern themselves only with what actually passes as knowledge in particular cases, while the epistemologists take care of what ought to pass as knowledge in general" (Fuller, 1988, p. 263).

A second example of interest here is gerrymandering in the study of academic information retrieval and exchange (Stoan, 1991). While sociologists study informal methods of research, such as "invisible colleges," librarians study researchers' use of formal research methods such as bibliographies, indexes, and abstracts. Neither type of study alone can explain why younger scholars rely more heavily on formal research sources yet make less use of formal sources as their careers progress. A unified approach could explain this, arguing that scholars are less closely tied to informal networks early in their careers and therefore are forced to rely more on formal sources of information. We do not have such an approach because of where the academic boundary is drawn.

Crossing these gerrymandered boundaries can play an important positive role in knowledge production. As scientists move outside their original scientific communities, social pressures are less constraining. This leaves scientists more open to conversion. While such conversion is not necessarily a sign of having adopted "true" beliefs, conversion that cuts across social pressures is better evidence of persuasiveness than conversion stemming from socialization within a particular discipline.

Similarly, informed observers in a different community are a useful judge of the likely validity of some set of scientific statements. Campbell (1994) gives the example of applied plant and animal breeders and doctors assembling family histories of specific disorders, both of whom were important for Mendelian genetics: "Because of their lack of prior commitment and lack of membership in partisan thought collectives, these groups have less social influence on them against adopting the new paradigm" (p. xviii). Similar kinds of arguments are to be found in more philosophical approaches, where the clash of rival paradigms or research programs (Kuhn, 1962; Lakatos, 1970) leads more or less to survival of the fittest.[13] The argument is directly analogous to Mills's belief in the free exchange of ideas.

Such arguments might lead one to conclude that crossing boundaries always makes innovative knowledges more likely (Dogan & Pahre, 1990), while remaining within boundaries always risks producing nontruthful knowledges. Alas, life is never so simple. Women's studies is a good illustration of some tensions evident in boundary crossing and in the cross-validation of multiple communities. Prior to the development of women's studies, existing academic organizations often excluded women and many issues important to feminists. Social factors such as sexism clearly played a role in shaping existing disciplines and in excluding research by, for, and about women. Just as clearly, scholarship responding to feminist challenges makes some knowledge claims that are clearly "more true" than the previous knowledge claims.[14]

For these reasons, women's studies intentionally challenged existing organizational forms, though it was far from clear what the organizational solution was (for brief histories see Boxer, 1982; Klein, 1990, pp. 95-98; Sheridan, 1990). Should feminist scholars build women's studies as an interdisciplinary field or should they work to transform existing disciplines from within? Either choice entailed boundaries and thus the implicit exclusion of something (Gunew, 1990, pp. 25-31). Each choice also has implications for scholars outside the boundaries—if women's studies became a separate department, other departments in the university might well remain unchanged by feminist scholarship.

As these examples show, even "interdisciplinary" fields have boundaries that are constructed and defended in some way. While they can transform knowledge, interdisciplinarity offers only temporary emancipation from boundaries. Understanding knowledge communities requires studying both the social forces behind boundaries and the truth-seeking efforts to transform them. This dynamic interaction between social constraint and truth seeking is central to the next section.

An Epistemological Sociology

Scientists are positivist, realist, and empiricist, and they believe that we can construct narratives that reflect objective reality. They believe that the purpose of science is to collect data, test hypotheses, and construct theories about the real world. Because scientific beliefs reflect reality, and scientific methods and theories help science better comprehend reality, these beliefs, methods, and theories must affect both scientific behavior and social organization. This claim is central to this author's epistemological sociology (Pahre, 1996) and Schmaus, Segerstrale, and Jesseph's (1992) "Hard Program in the Sociology of Scientific Knowledge" (see also Fuller, 1988, pp. 263-75).

Of course, social factors both internal and external to science also affect these communities, so this approach is not simply yet another example of a (discredited) normative sociology (see Collins, 1992). To see how this works, consider a commonly cited example—multidisciplinary teams doing "problem-focused research." This is (applied) research on a particular problem, usually a social problem of some sort. Understanding where these problems originate requires some sociological understanding of researchers' connections to the outside world. Yet the justification for multidisciplinary teams is epistemological, an argument that we cannot solve problems that straddle several disciplines without seeking true information from each (Ben-David, 1973; de Bie, 1970; Heckhausen, 1972). This is especially true, the argument runs, for clinical care (Klein, 1990, pp. 140-55). The social need for a certain kind of knowledge, combined with the "epistemological" rules for pursuing this kind of multidisciplinary knowledge, combine to produce a particular form of knowledge community, the multidisciplinary team.

Next we will turn to an extended example of where the epistemological sociology approach can take us. We will begin with the claim that disciplines are organized according to their dependent variables, which make up the facts with which their discipline is concerned.[15] This is true (1) for social reasons, because society wants certain sets of problems solved or facts explained; and (2) for epistemological reasons, because scientific norms lead scientists to explain facts, where they know the facts in advance but not the explanations. Once organized into disciplinary communities, scientists develop hypotheses and theories to explain the facts that their community studies.

Given this construction of disciplines, any theory claiming to be useful to a discipline must be *germane* to that discipline—i.e., it must explain some subset of the new discipline's existing set of data. Sociologists will not borrow the apparently true statements of quantum mechanics from physics unless these statements say something about sociological data. Social forces arise to defend boundaries, so "facts" will persist longer within a knowledge community than either "hypotheses" or "theories" (compare Campbell & Paller, 1989, p. 242)—facts are more essential to the maintenance of these boundaries.

There arise, then, multiple scientific communities, each seeking hypotheses and theories to explain the facts in their disciplines. Some of these sets are likely to overlap for two reasons. First, any system of classifying facts is a social construction. One good example is the different taxonomies found in Western science and among the Karam aborigines (Barnes, 1983). The West classifies bats as mammals and cassowaries as birds; Karam classifies (flightless) cassowaries as animals and (flying) bats as birds. Karam makes the "flying" characteristic central to its schema, while the West classifies according to genetic or evolutionary relationships. Both taxonomies are equally logical, so social needs decide which taxonomy governs—genetic relationships being most important to the West, behavioral characteristics to the Karam. Because classification schemata are (epistemologically) arbitrary, there is no reason why a given object need belong exclusively to a single discipline. For these reasons, we find many dependent variables to be germane in more than one discipline. Klein, in this issue of *Library Trends*, gives examples of crime (economics, sociology, political science, and others), poverty (economics, sociology, political science, women's studies), and disease (medicine, public health).

A second reason several disciplines explain some of the same facts is that any fact is open to multiple interpretations by the Duhem-Quine thesis. Multiple explanations of the same fact are ubiquitous in science—for instance, the choice between two or more plausible explanations is at the heart of most scientific controversies. It is less frequently remembered that there can be not only multiple interpretations of the same facts, but multiple consistent logical systems—such as non-Euclidian geometries—each incompatible with the other. Scientific controversies need not end with one interpretation victorious but may produce two or more internally consistent bodies of theory.[16]

The appearance of the same "fact" in more than one discipline is an obvious inducement to interaction between fields. Let us consider two forms of interaction: (1) the exchange of data detached from theory, and (2) the exchange of hypotheses designed to explain some data.

As epistemological sociology would expect, the epistemological difference between data and hypothesis leads to different kinds of community. Borrowing data, for instance, does nothing to create community between disciplines. The same data can appear in multiple fields without changing them at all, no matter where the data came from. The common use of economic data does not create a community between political scientists and economists or between sociologists and economists.

The exchange of hypotheses, on the other hand, can create community. Because the sets of objects studied by different disciplines overlap, scientists always have an inducement to exchange hypotheses. This follows directly from scientists' beliefs about what they are doing and can take several different forms. First, one discipline might borrow another discipline's hypotheses and use them to explain some data in the borrower discipline not found in the lender discipline. For instance, in the 1950s and 1960s, many anthropologists and political scientists found Parsonian sociology useful and borrowed this theory for their own purposes. Second, any lender discipline that observes this borrowing might find the borrower discipline's facts interesting and might simply incorporate any data into its own field that these hypotheses explained in the borrower field. For instance, there are now economic explanations of family structure so the discipline of economics now includes data about families and society without such data being excluded from sociology.

This appropriation of data might make part of the borrower field appear as an outgrowth of the lender field. When this occurs, observers will describe an "imperialist" discipline that enters the subject matter of neighboring fields. Political economy is a good example, driven by the theoretical imperialism of economics (Hirshleifer, 1985). A sizable economic literature now explains economic regulation, a topic originally germane to political science. By this process of incorporation, economic policies have become part of the explanandum of economics, which has expanded to include them. Economic fields such as the "economics of regulation" or "endogenous tariff theory" now explain politics in economic terms.

Because facts define a scientific field and hypotheses are tightly linked to facts, borrowing results can also lead directly to the creation of a new scientific field. There are two possibilities here: the two fields may merge entirely, as did botany and zoology in 1945-1955, or the exchange of results may lead to the creation of a new hybrid field that defines itself by the facts at the interstices of the parent disciplines.

Perhaps the largest hybrid field is historical sociology, where sociology and history interact to produce results that are useful to the other (see Dogan & Pahre, 1990, pp. 187-201). The community is so large that there exists subcommunities depending on different kinds of ties to the outside. For instance, the school of the *Annales* pursues multidisciplinary studies of the *long durée.* Grounded in communities of historians, this school attempts to use social scientific and natural scientific results as part of the reconstruction of particular historical social structures.

It is easy to multiply such examples because the exchange of hypotheses is a bread-and-butter form of interaction across scientific boundaries (see Campbell, 1969; Dogan & Pahre, 1990; Klein, 1990). It changes the boundaries of existing fields and may lead one field to subsume another or two fields to merge into a hybrid. Though closely tied to data, this is very different from the mere exchange of data, with different implications for community. The exchange makes perfect sense in terms of scientific beliefs in positivism and realism, according to which scientists seek explanations of reality in a variety of places.

There are also borrowings entirely divorced from data. For instance, it is possible to borrow assumptions and deductions derived from them completely detached from facts. A borrowing detached from facts is especially evident when the theory is couched in mathematical terms and a field borrows only the mathematical terms (see also Pahre, 1996). An interesting example is the borrowing of Newton's inverse-square law by international trade theorists in economics. Jan Tinbergen (1962) and Pentti Pöyhönen (1963) saw this as a useful way to estimate trade volumes between countries. Their initial insight was to relabel the algebra and create a "gravity model" of international trade in which force is reinterpreted as bilateral trade volume, mass becomes the GNP of any two countries, and the distance between these countries has an inverse-square effect on trade. These "gravity models" describe trade flows better than any other theory we know, despite having no connection at all to economic theories of international trade (see Leamer & Stern, 1970, chap. 6; Deardorff, 1984).

Such borrowings rest on the language of metaphor. Metaphorical communities may emerge, but successful innovation in a metaphorical community is a less predictable matter. Certainly physicists and economists will not find any ground for a community in the above example. The difficulty of forming metaphorical communities is inexplicable in terms of constructivist sociology but makes sense for epistomological sociology because it relies on distinctions among data, hypothesis, and mathematical language that are important to scientific epistemology.

The exchange of research methods also does little to create community. Statistical methods are a good example. Any concept or method from the field of statistics—i.e., the description of a Gaussian distribution, sampling rules, Bayesian inferences, and hundreds more—is substantively empty; it matters not whether one is counting gold mine production, deaths in war, or quasar emissions. In other words, such methods are not at all linked to facts. Yet such concepts, and the methods for applying or manipulating them, are enormously useful and have spread from discipline to discipline. Despite their importance, they do not create communities or disciplines, which are always organized around dependent variables.

Let us conclude this section by contemplating some implications of epistomological sociology for information science. Facts are stable and central to the construction of disciplinary boundaries. Designing information systems that respect these disciplinary boundaries makes sense. Still, information systems are themselves social constructions and help strengthen these boundaries, not challenge them.

Facilitating the exchange of hypotheses is a matter of helping communication among "neighbors." While there are likely to be many problems in practice, in principle it is easy to see that monetary policy will interest economists and political scientists, peasant villages will interest anthropologists and sociologists, and speech recognition will interest linguists and psychologists. The relevant communities can help information providers recognize these needs; though research in psycholinguistics might be classified as part of either psychology or linguistics, researchers need hardly be told to look in both places for recent contributions to the field.

The primary obstacles to information science's efforts to cultivate the exchange of hypotheses are likely to be social. Hybrid fields conflict with socially constructed boundaries that are embedded in fiscal constraints, administrative divisions, and academic politics (see Searing's article in this issue of *Library Trends*). When forced to choose between allocating resources to facilitate research at the interstices of disciplines or giving resources to support research at the discipline's core, most administrators will emphasize the core.

In contrast, the major obstacles to metaphorical communities are epistemological. We are not very used to thinking about them, and it is hard to imagine how information science might facilitate exchange in this area. Complexity theory, also known as chaos theory (see Gleick, 1987), is a good example of these difficulties. The core of this cluster of theories is the methodological principle that apparently chaotic behavior can be patterned in complex ways, and that we should model this behavior from the "bottom up" and then look for patterns. For instance, some lines of computer code might simulate the rules guiding an ant, a migrating bird, nations at war, protein synthesis, or a weather system. The computer can then simulate how a large number of these units would behave in interaction with each other, and the scientist can scan this behavior for patterns such as flocking behavior in birds or the way that ants sort different kinds of trash.

What makes complexity theory a challenge is the diverse applications possible with this method (my seemingly random list above is taken from real research). A recent graduate course on complexity theory in my political science department had students from chemistry, computer science, mathematics, psychology, and political science; colleagues in this area interact with biologists, economists, sociologists, and many other disciplines. There is no obvious way to connect a would-be user in the study of war to information such as the Lorenz equations, originally written to simulate the behavior of a water wheel.

Conclusions: Toward an Epistemological Sociology Research Program

While there are many challenges for information science raised by the study of crossdisciplinary communication, this discussion will conclude with what this author sees as the research program facing those doing epistemological sociology. After some years of research, we should then be in a much better position to think normatively about issues of concern to universities, science policy makers, and information service providers.

First, this research program needs a more fully developed epistemology. Institutional and social organization, social-psychological pressures, politics, and economic incentives all shape the pursuit of knowledge, and social epistemology is well poised to explain how such social processes help or hinder efforts to develop a better understanding of the real world. A foundationalist epistemology cannot explain changing constructions of truth-seeking disciplines and their boundaries in the same way that a social epistemology can. These changing constructions are, in turn, an important source of sociological change in an epistemological sociology.

The second task for epistemological sociology is to develop a large body of hypotheses about how epistemology shapes scientific organization. The ES reaction to constructivism risks making the following kind of argument: scientific norms matter, so if we observe scientists we should see them seeking truth in accordance with those norms. There are two problems here. First, the argument does not add any information to our understanding of science since we have assumed norms in order to explain norm-driven behavior. More seriously, any empirical study based on such an argument will likely be tautologous, deriving the norms only from the study of normative behavior and then using these derived norms from the very same behavior.

To avoid these problems, hypotheses are proposed here that connect norms with social organization, mostly about cross-boundary communities.[17] These hypotheses should be compared to existing models of these communities, such as specialization-fragmentation-hybridization (Dogan & Pahre, 1990), or a spatial model of islands and archipelagoes (Berger, 1972; Garfield & Small, 1985). These models are not mutually exclusive, but we have not yet asked under what circumstances a particular model will fit one field or another. It is at least as important to start asking which models do not fit particular fields and which models do not seem to fit very many fields at all.

As this suggests, it is time for sociologists of science to buckle down and pit contending approaches against each other in empirical tests. Warren Schmaus et al. (1992; also compare Collins, 1992) note that "social students of science do not seem to think it necessary to eliminate alternative explanations and demonstrate the superiority of their own explanation; they just argue their own specific case" (p. 249). For those of us who grant that theory testing is meaningful—as positivists or as social epistemologists—this lack of testing is an undesirable state of affairs. Because these tests should be comparative—that is, against a rival paradigm or research program (Kuhn, 1962; Lakatos, 1970)—it is helpful to pose epistemological sociology against the constructivist sociology of science.[18]

Another way to develop hypotheses is to treat norms not as a constant that structures society but as a variable that influences the structure of scientific society. For instance, scientific norms about the purpose of research are much stronger than norms about the purpose of making university appointments (where scientific ability and nonscientific norms such as teaching, mentoring, or diversity all play a role). We should also expect the effects of scientific norms to be more obvious in crossdisciplinary science. Very powerful social forces, crystallized as disciplinary boundaries, may well overwhelm these norms as an explanation of disciplinary and intradisciplinary organization.

Whatever the details, this is an exciting area of research. Looking at both the social constraints on knowledge and the way that knowledge transforms communities forces the scholar to be reflexive and self-critical. At the same time, this research also highlights the creative and transformative potential latent in existing social structures and communities.

NOTES

1 Similarly, connecting information to end-users is not just a technological problem but a sociological one. However, librarians typically think about meeting the needs of information users in terms of technological fixes—better abstracting, indexing, online search capabilities—instead of social solutions.

2 Throughout this paper I use "epistemology" as a shorthand that includes much that is not only epistemological but also ontological or hermeneutical. For instance, this "epistemology" also includes scientists' ontological beliefs that reality exists and hermeneutical guidelines about uncovering the secrets of that reality. I also use the term to include the process of dividing the scientific toolkit into "data," "methods," "theory," and other more-or-less exclusive categories.

3 Another implication of the sociological study of disciplines stems from the fact that members of the different divisions (engineering, humanities, natural sciences, social sciences) search for information in different ways. While all rely heavily on informal networks, bibliographic searches are much more important in the natural sciences, while library accession lists and publishers' catalogs are much more important in the humanities (Stoan, 1991). Thus, studying scientific communities is important not only in order to understand the informal information sources that stand as alternatives to librarians' formal sources, but also to understand the origins and likely persistence of the differences in the use of formal sources. These issues will not be addressed here.

4 For seven examples of failed foundationalist attempts to mark off the human sciences, see Fuller (1988, pp.197-201). For review of the argument distinguishing knowing from unknowing objects, see Harbers & de Vries, 1993 and Lynch, 1993.

5 Laudan's (1977) critique of those who try to demarcate science from non-science is telling, for each philosopher tried to design criteria to exclude specific beliefs that he finds objectionable: Aristotle excluded Hippocratic medicine, Carnap ruled out Bergsonian metaphysics, and Popper put Freud and Marx beyond the Pale.

6 Fuller (1988), too, notes that "history tells against the systematic approach" to organizing disciplines (p. 196).

7 For an example of how a highly critical external audience shapes research in the humanities, see Messer-Davidow (forthcoming).

8 We must also consider the interests of "users" (Fuller, 1987, pp. 157-58), such as those who accept (use) claims about materials, forces, and stress in order to assert claims about architecture. It is impossible to explain the persistence of knowledge claims in the face of social change without considering a broad range of such "interests."

9 Numerous variations exist within this general position, including constructivists (Knorr-Cetina, 1983), discourse analysts (Mulkay et al., 1983), ethnomethodologists (Lynch et al. 1983), postmodernists, and the like. I take this term to include a large cluster of post-modern, post-structuralist, ethnomethodological, deconstructing, and discourse-oriented perspectives, despite the myriad differences among these sects (see Knorr-Cetina & Mulkay, 1983).

10 For nonconstructivist discussions of the social nature of disciplines, see, among others, Bauer (1990), Becher (1990, 1994), Campbell (1979), Pinch (1990).

11 Donald T. Campbell (1969, 1986, 1989, 1994) calls this project a "Sociology of Scientific Validity" (SSV), while Goldman uses the term "veritism" for the evaluation of social practices according to their production of true beliefs. The project is also central to the journal *Social Epistemology* and editor Steve Fuller's (1988) book of the same name, and I have followed the nomenclature of that community here.

12 It might also be interesting to think about the political aims of the sociology of science: (1) to debunk the achievements of the natural sciences in order to make the natural sciences resemble the social sciences, who would then share in the higher prestige of the natural sciences; (2) to buttress the position of sociology as a discipline capable of understanding "reality" objectively, and thus something different than the less prestigious subjective disciplines of the humanities.

13 Social epistemology need not be this panglossian, of course. Compare Fuller's (1988) statement of the task: "[M]ost of the cognitive utopias of the philosophers involve activities such as inspecting the logical structure of arguments and replicating the experiments of one's colleagues, which are simply impossible to enforce on a systematic basis in the world of Big Science" (p. 268). Thus, understanding the social constraints on replication is a necessary condition for a normative epistemology.

14 For a trite example, consider those newer truth claims in medical studies that are drawn from the population of both women and men. These are an advance on pre-existing studies, which generally excluded women from the sample even for studying medical problems suffered mostly by women.

15 Throughout this essay I will treat "facts," data, and objects as unproblematic and as somehow prior to theory. This is a simplification, to say the least. Someone "discovers" certain facts for certain purposes and not some other imaginable facts, and we describe these facts in one language and not another.

16 If internally consistent, each must be incomplete, by Gödel's Theorem.

17 For a different kind of example, in which the interaction of truth-seeking and citation maximization goals produce particular patterns of replication in high and low status journals, see Feigenbaum and Levy (1993).

18 This is not quite a fair test because constructivists reject the positivist project of theory testing.

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